a difference which has arisen at some time through a variation? We can determine characters only by comparing related organisms and noting their differences. To say that all characters are alike, then, is to say that all variations are alike, which Sir Archdall Reid himself admits is not the case.

Perhaps an experimental instance will make this clearer. Some years ago a fasciated specimen of an Œnothera was sent to me. The plant was in seed; the stem was about 2 in. wide at the widest part and as flat as a ribbon. It was, of course, impossible to say with certainty, from inspection, whether this character would be inherited or not, although the probabilities were somewhat against it. I sowed the seeds, large numbers of them, and they all gave rise to perfectly normal plants with round stems. The character was therefore non-inherited in this particular case. It is, of course, well known that fasciations may be produced by excessive nutrition, and that the peculiarity is then, as a rule at least, not inherited.

But there are other instances in which this character *is* inherited. For example, in the common coxcomb of gardens, *Celosia cristata,* fasciation is one of the specific characters, distinguishing it from such species as *Celosia plumosa*, in which the stems do not fasciate under ordinary conditions of cultivation. I have often grown these two species in quantity side by side in the greenhouse, and compared the extreme fasciation of *C. cristata* with the ordinary branched character of the other species. It should be mentioned, however, that *C. plumosa* does sometimes show slight fasciation at the tips of the branches, and this can be exaggerated by growing the plants under conditions of very high temperature and moisture. But it never approaches the degree of fasciation found constantly as a specific (and therefore inherited) character in *C. cristata*.

The same character, fasciation, is therefore clearly inherited in *C. cristata,* but it was not inherited in the particular instance in Œnothera which I tested. It is also clear that the fasciated Celosia must have originated at some time as a variation from plants with normal stems. Innumerable similar instances will be known to experimental biologists, and it is such cases which they have in mind when they speak of characters as of two kinds, inherited and non-inherited. When a particular new character appears as the result of a variation no one can predict with certainty whether it will be inherited or not until the organism which shows it is tested. But, of course, probabilities may be stated by comparison with similar characters the hereditary behaviour of which is already known. In the face of such experimental facts, which are well known to all geneticists, it is futile to state that all characters are equally acquired and equally inherited.

When Sir Archdall Reid implies that combs and corns are equally inherited he forgets a whole class of experimental facts such as those above cited. One must refuse to consider corns as inherited, because there always remains the possibility that a case may arise where, through a germinal change, they *are* inherited without any special stimulus to produce them. The inherited condition known as keratosis is, indeed, an epidermal thickening of similar character. It seems clear that moles are not usually inherited, but if the writer in NATURE is correct (see NATURE, January 19, p. 78), then there may be instances in which even a mole is inherited in the legitimate sense in which the term "inheritance" is customarily used by biologists.

R. RUGGLES GATES.

King's College, University of London,
January 27.

SIR ARCHDALL REID'S letter in NATURE of January 26 will render considerable service if it induces students of evolutionary phenomena accurately and precisely to define their terms.

If one may, at the beginning, set forth two general statements, the ground will be cleared for a discussion of Sir Archdall Reid's points:—

(1) Genes or factors are inherited, characters are not.

(2) A gene conditions the appearance in the organism of a character or group of characters.

(3) The effect produced by a gene in the organism depends on the environmental conditions which prevail during the life-history of the organism and on the other genes which the organism possesses.

To show that characters are not inherited, the example of "abnormal abdomen" in Drosophila may be cited.

The gene for "abnormal abdomen" causes the condition in moist cultures only. In dry cultures the flies hatch out normal in appearance.

The statement that rose comb and single comb are not more inheritable than corns on oarsmen's hands is obviously correct. Any capacity for reacting to a stimulus may be considered as being represented in the chromosomes by a gene or genes. In this case we may assume that the capacity for responding to the frictional stimulus of the oar by forming a mass of proliferated tissue on the palms of the hands is inherited.

Certain other points raised by Sir Archdall Reid may be dealt with briefly:

(1) The impure dominant does not inherit any trait. It inherits the recessive gene from one parent which may or may not interact with environment and with other genes to produce an effect. The terms "dominant" and "recessive" are purely arbitrary, and used only for convenience.

(2) The pure extracted recessive inherits a recessive gene from one parent and a similar recessive gene from the other. The germ-cells of an impure dominant carry either the dominant or the recessive gene.

(3) The ancestral condition obtained in some pigeon crosses is due to the interaction of the two sets of genes contributed by the two parents.

The interaction of genes may be illustrated by an example from the cow-pea. A red cow-pea crossed with a white may give a black in the first hybrid generation. White possesses a gene for black which is without effect except in the presence of the gene for red present in the red parent. At least eight different genes in the cow-pea are known to depend for their expression on a single colour-conditioning gene.

S. C. HARLAND.

[Sir Archdall Reid began this correspondence with a letter in NATURE of November 25, 1920; and we have now invited him to close it.—EDITOR.]

The Radiant Spectrum.

DR. HARTRIDGE'S objections to my explanation of this phenomenon (NATURE, September 1, p. 12, and December 8, 1921, p. 467) seem to be based on an imperfect appreciation of Brewster's observations on the subject. Brewster brings out two facts clearly in his paper: First, when a very small and intense source of white light is viewed directly by the eye it appears surrounded by a system of radiating streamers which appear to diverge directly from it; secondly, when a prism of small dispersive power is interposed in front of the eye the streamers are deviated and now appear to diverge from a point lying beyond the violet end of the spectrum into

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which the source itself is drawn out. It is clearly illogical to suggest, as Dr. Hartridge does, that the prism is responsible for the radiant phenomenon in view of the fact that, in its essential features, the effect is observed even before the introduction of the prism.

Using a sufficiently intense source of light and a prism of small angle with optically good and clean faces, and making the observations in a dark room, it should be easy for anyone to satisfy himself by simple tests of the kind referred to by Dr. Hartridge that he is in error, and that Brewster's phenomenon really arises from the scattering of light in the eye, the prism merely acting as a dispersive apparatus modifying the colour and disposition of the streamers in the halo surrounding the source. Judging from the statements made in his letter, Dr. Hartridge would appear to have been particularly unfortunate in his choice of experimental conditions. Any noticeable imperfection in the optical surfaces of the prism would, of course, give rise to scattering, masking the true phenomenon due to the eye itself. This is indeed clearly suggested in Brewster's own paper.

A further and absolutely crucial test is also available. In my paper on the scattering of light in the refractive media of the eye (*Phil. Mag.*, November, 1919, p. 568), I have described the character of the diffraction-halo arising from this cause in considerable detail. With a source of white light the halo shows a radiating fibrous structure and clearly marked alternations of colour and intensity in its outer parts. A monochromatic source, on the other hand, exhibits a halo with a granular structure and a succession of bright and dark rings. These features are explained in my paper as due to the diffraction of light by corpuscles of more or less uniform size included within the structure of the eye. On this view we should expect one half of the first diffraction ring outside the central portion of the halo to be partially achromatised on the introduction of the prism and to appear as a detached semi-circular arc lying beyond the violet end of the spectrum and the displaced position of the achromatic centre. No mere imperfections or irregularities in optical surfaces could, on the other hand, give rise to such a phenomenon. Actual trial confirms the expectation from theory and puts its correctness on an unassailable basis.

C. V. RAMAN.

210 Bowbazaar Street, Calcutta, January 4.

The Naming of the Minor Planet No. 907, Barnardiana.

IN NATURE for September 8 last (vol. 108, p. 69), at the end of "Our Astronomical Column," attention is directed to the naming by Dr. Max Wolf of two of his asteroids in *Astronomische Nachrichten*, No. 5116. They are No. 834, Burnhamia, and No. 907, Barnardiana. In commenting on these asteroids NATURE infers that they were named after two American astronomers. While it is true that Prof. Burnham's memory is thus honoured, Barnardiana was not named after me, but in memory of Mrs. Rhoda Calvert Barnard, who died on May 25, 1921. This is evident from the following quotation from a letter to me by Dr. Wolf on the subject:—

"Wenn ich den Vornamen Ihrer Gemahlin gekannt hätte, und—vorausgesetzt, dass er nicht schon verwendet worden ist—würde ich ihn einem meiner Planeten zur Erinnerung an Ihre liebe Frau beigegeben haben. Da das nicht ging, so taufe ich den Planeten 907 1918 EU, auf den Namen: Barnardiana."

For some reason No. 5116 of the *Astronomische Nachrichten* containing these names has only very recently reached the Yerkes Observatory. I was unaware until then that it did not distinctly state the planet was named after Mrs. Barnard. Though not actively engaged in astronomical work, in her long life in astronomy she had endeared herself to the many astronomical people she had met by her thoughtful and unselfish interest in them and in their work. Hers was a life of love and sympathy. I am grateful to Dr. Wolf for thus perpetuating her memory.

E. E. BARNARD.

Yerkes Observatory, University of Chicago,
January 11.

The Resonance Theory of Hearing.

DR. HARTRIDGE imputes to me great absurdities which, either in irony or by an excess of courtesy, he terms "slight errors" (NATURE, January 19, p. 76). Under (1) he takes my plain words, the result "must always be of the same nature," to mean that the result must always be the same! Of course, the harmonic analysis of his oboe and flute combination will not give the same result as in the case of violin and cornet, but in both cases the result will be of the same nature, in that there will be only one fundamental tone. If the data supplied to the sensorium from the cochlea are simply the result of an harmonic analysis, the two notes must appear to the ear inseparably blended in one note. I have not left binaural audition out of consideration. The ability to distinguish two concurrent notes of the same pitch and different quality seems unaffected by both sources being equidistant from either ear.

Under (2) Dr. Hartridge should know as well as I know that the *pitch* of a note depends solely upon the period of its fundamental tone. The example which I proposed eliminates the possibility of beats, the two notes being in perfect physical unison. And, further, since the note made by the teeth is generated by the other note, it cannot be heard except in the combination. Its perception is, therefore, a cognition, not a recognition. *At any instant* during the production of the two notes (which may be sustained for twenty seconds easily) it is possible to turn the attention to the note made by the teeth and to hear that its pitch is that of the hummed note. *At no instant* could the resonators which Dr. Hartridge, outstripping Sir Arthur Keith, "finds" in the cochlea furnish the data for anything but a change in the quality and intensity of the hummed note. This objection remains untouched by Dr. Hartridge's animadversions. It goes to the root of the matter, and cannot "fall to the ground" as a superstructure may.

W. PERRETT.

University College, Gower Street, W.C.1,
January 26.

Aurora Borealis of January 30.

HAPPENING to look out at 11.30 last night I perceived a strong auroral glow extending from N. by E. through N. to W. The light was quite bright, and on going into the garden I noticed that my body cast a shadow and that I could read the headlines of the *Times* quite readily. There were no streamers, but several luminous patches, especially due N., where a blunted cone of greenish light rose vertically up from the horizon to a height of 10°.

The sky was partially, and later almost totally, covered by thin clouds, which drifted up from S. under the influence of light airs. The atmosphere was misty and the temperature decidedly warm.

CHARLES S. LEAF.

7 Grange Road, Cambridge. January 31.

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